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XIII. *The supposed Effect of boiling upon Water, in disposing it to freeze more readily, ascertained by Experiments.*

By Joseph Black, M. D. Professor of Chemistry at Edinburgh, in a Letter to Sir John Pringle, Bart. P. R. S.

TO SIR JOHN PRINGLE, BART. P. R. S.

DEAR SIR,

Edinburgh, Feb. 11, 1775.

Redde, Feb. 23,
1775.

WE had lately one day of a calm and clear frost; and I immediately seized the opportunity, which I missed before, to make some experiments relative to the freezing of boiled water, in comparison with that of water not boiled. I ordered some water to be boiled in the tea kettle four hours. I then filled with it a Florentine flask, and immediately applied snow to the flask until I cooled it to 48° of FAHRENHEIT, the temperature of some unboiled water which stood in my study in a bottle; then putting four ounces of boiled, and four of the unboiled water, separately, into two equal tea cups, I exposed them on the outside of a north window, where a thermometer pointed to 29° . The consequence was, that ice appeared first upon the boiled water; and this, in several repetitions of the experiment, with the same boiled water, some of which

which were made nine hours after it was poured out of the tea kettle. The length of time which intervened between the first appearance of ice upon the two waters was different in the different experiments. One cause of this variety was plainly a variation of the temperature of the air, which became colder in the afternoon, and made the thermometer descend gradually to 25° . Another cause was the disturbance of the water; when the unboiled water was disturbed now and then by stirring it gently with a quill tooth-pick, the ice was formed upon it as soon, or very nearly as soon, as upon the other; and from what I saw, I have reason to think, that were it to be stirred incessantly, provided at the same time the experiment were made with quantities of water, not much larger or deeper than these, it would begin to freeze full as soon. In one of these trials, having inspected my tea cups when they had been an hour exposed, and finding ice upon the boiled water, and none upon the other, I gently stirred the unboiled water with my tooth-pick, and saw immediately, under my eye, fine feathers of ice formed on its surface, which quickly encreased in size and number, until there was as much ice in this cup as in the other, and all of it formed in one minute of time, or two at most. And in the rest of the trials, though the congelation began in general later in the unboiled water than in the other; when it did begin in the former, the ice quickly encreased so as, in a very short time, to equal, or nearly equal in quantity, that which had been formed more gradually in the boiled water. The opinion,

therefore, which I have formed from what I have hitherto seen is, that the boiled and common water differ from one another in this respect; that whereas the common water, when exposed in a state of tranquillity to air that is a few degrees colder than the freezing point, may easily be cooled to the degree of such air, and still continue perfectly fluid, provided it still remain undisturbed: the boiled water, on the contrary, cannot be preserved fluid in these circumstances; but when cooled down to the freezing point, if we attempt to make it in the least colder, a part of it is immediately changed into ice; after which, by the continued action of the cold air upon it, more ice is formed in it every moment, until the whole of it be gradually congealed before it can become as cold as the air that surrounds it. From this discovery it is easy to understand, why they find it necessary to boil the water in India, in order to obtain ice. The utmost intensity of the cold which they can obtain by all the means they employ, is probably not greater than 31° or 30° of FAHRENHEIT'S thermometer. Common water, left undisturbed, will easily descend to this degree without freezing; and, if they have not the means of making it colder, may continue fluid for any time, provided it be not disturbed: the refrigerating causes of that part of the world when they have done so much, have done their utmost, and can act no further upon the water. But this cannot happen to the boiled water; when the refrigerating causes have cooled it to 32° , the next effect they produce, is to occasion in it the beginning of congelation, while
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the water is afterwards gradually assuming the form of ice, we know, by experience, that the temperature of it must remain at 32° ; it cannot be made colder, so long as any considerable part of it remains unfrozen^(a). The refrigerating causes continue, therefore, to have power over it, and to act upon it, and will gradually change the whole into ice, if their action be continued sufficiently long.

The next object of investigation may be the cause of this difference between the boiled and the common water. In considering this point, the following idea was suggested. As we know from experience, that by disturbing common water, we hasten the beginning of its congelation, or render it incapable of being cooled below 32° , without being congealed; may not the only difference between it and boiling water, when they are exposed together to a calm frosty air, consist in this circumstance: that the boiled water is necessarily subjected to the action of a disturbing cause, during the whole time of its exposure, which the other is not? One effect of boiling water long, is to expell the air which it naturally contains; as soon as it cools, it begins to attract and absorb air again, until it hath recovered its former quantity; but this probably requires a considerable time. During the whole of this time, the air entering into it must occasion an agitation or disturbance in the water, which, though not sensible to the eye, may be very effectual in

(a) Common water, when cooled in a state of tranquillity to several degrees below the freeing point, will suddenly rise up to it again, if disturbed in such a manner as to occasion in it a beginning of congelation.

preventing it to become, in the least, colder than the freezing point, without beginning to freeze, in consequence of which its congelation must begin immediately after it is cooled to that point. When I reflect upon this idea, I remember a fact which appears to me to support it strongly. FAHRENHEIT was the first person who discovered that water, when preserved in tranquillity, may be cooled some degrees below the freezing point without freezing. He made the discovery while he was endeavouring to obtain ice from water that had been purged of its air: with this intention he had put some water into little glass globes, and having purged it of air, by boiling and the air-pump, he suddenly sealed up the globes, and then exposed them to the frosty air. He was surprized to find the water remain unfrozen much longer than he expected, when at last he opened some of his globes, in order to apply a thermometer to the water, or otherwise examine what state it was in. The immediate consequence of the admission of the air was a sudden congelation which happened in the water; and in the rest of his globes, a similar production of ice was occasioned by shaking them. The inference that may be drawn from these experiments of FAHRENHEIT's is sufficiently obvious; it appears to me to remove all doubt with regard to the above supposition. Before these experiments of FAHRENHEIT occurred to my memory, I had planned a few, suggested by the above supposition, that might have led to the same conclusion; but the short duration of the frost, for one day only, did not give me time to put them in execution.